Figure 2a. Plot of cumulative abnormal return for earning announcements from event day -20 to event day 20. The abnormal return is calculated using the market model as the normal return measure.

eraged across the 600 event observations (30 firms, 20 announcements per firm) as well as the aggregated cumulative abnormal return for each of the three earnings news categories. Two normal return models are considered; the market model and for comparison, the constant mean return model. Plots of the cumulative abnormal returns are also included, with the CAR's from the market model in Figure 2a and the CAR's from the constant mean return model in Figure 2b.

The results of this example are largely consistent with the existing literature on the information content of earnings. The evidence strongly supports the hypothesis that earnings announcements do in-

deed convey information useful for the valuation of firms. Focusing on the announcement day (day 0) the sample average abnormal return for the good news firm using the market model is 0.965 percent. Given the standard error of the one day good news average abnormal return is 0.104 percent, the value of θ_1 is 9.28 and the null hypothesis that the event has no impact is strongly rejected. The story is the same for the bad news firms. The event day sample abnormal return is −0.679 percent, with a standard error of 0.098 percent, leading to θ_1 equal to −6.93 and again strong evidence against the null hypothesis. As would be expected, the abnormal return of the no news firms is small at −0.091 percent and
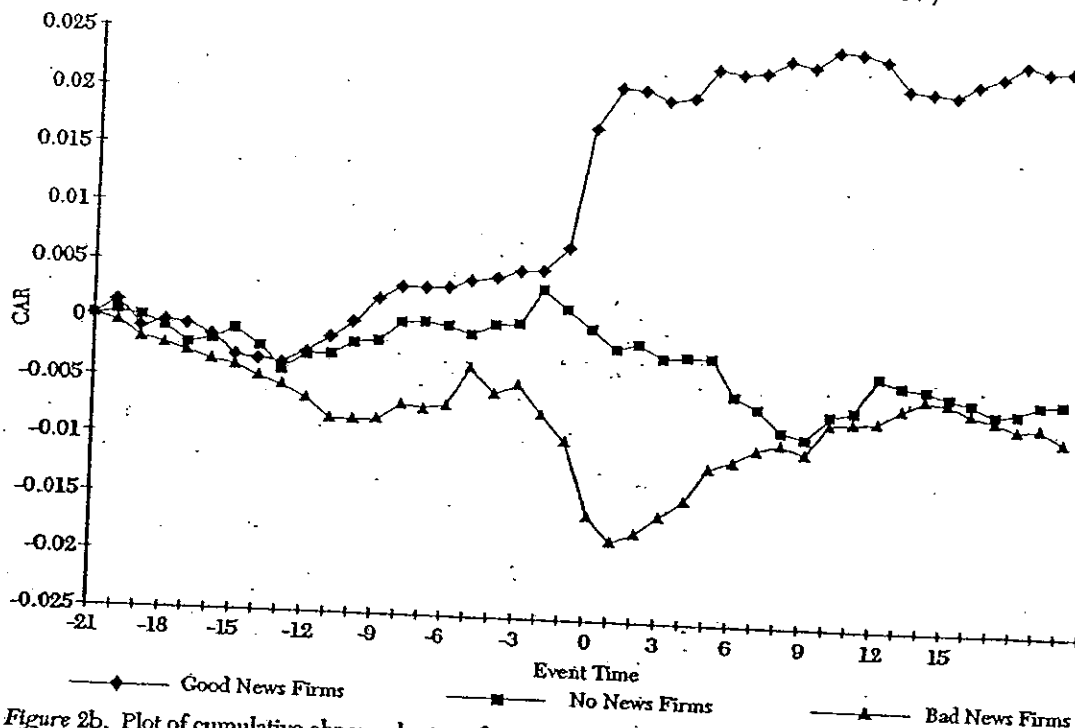
Figure 2b. Plot of cumulative abnormal return for earning announcements from event day -20 to event day 20. The abnormal return is calculated using the constant mean return model as the normal return

with a standard error of 0.098 percent is less than one standard error from zero. There is some evidence of the announcement effect on day one. The average abnormal return is 0.251 percent and −0.204 percent for the good news and the bad news firms respectively. Both these values are more than two standard errors from zero. The source of these day one effects is likely to be that some of the earnings announcements are made on event day zero after the close of the stock market. In these cases, the effects will be captured in the return on day one.

The conclusions using the abnormal returns from the constant return model are consistent with those from the market model. However, there is some loss of precision using the constant return model, as the variance of the average abnormal return increases for all three categories. When measuring abnormal returns with the constant mean return model the standard errors increase from 0.104 percent to 0.130 percent for good news firms, from 0.098 percent to 0.124 percent for no news firms, and from 0.098 percent to 0.131 percent for bad news firms. These increases are to be expected when considering a sample of large firms such as those in the Dow Index because these stocks tend to have an important market component whose variability is eliminated using the market model.

The CAR plots show that to some extent the market gradually learns about the forthcoming announcement. The average CAR of the good news firms gradually drifts up in days −20 to −1 and the average CAR of the bad news firms gradually drifts down over this period. In the days after the an-

nouncement the CAR is relatively stable as would be expected, although there does tend to be a slight (but statistically insignificant) increase with the bad news firms in days two through eight.

E. *Inferences with Clustering*

The analysis aggregating abnormal returns has assumed that the event windows of the included securities do not overlap in calendar time. This assumption allows us to calculate the variance of the aggregated sample cumulative abnormal returns without concern about the covariances across securities because they are zero. However, when the event windows do overlap and the covariances between the abnormal returns will not be zero, the distributional results presented for the aggregated abnormal returns are no longer applicable. Victor Bernard (1987) discusses some of the problems related to clustering.

Clustering can be accommodated in two ways. The abnormal returns can be aggregated into a portfolio dated using event time and the security level analysis of Section 5 can applied to the portfolio. This approach will allow for cross correlation of the abnormal returns.

A second method to handle clustering is to analyze the abnormal returns without aggregation. One can consider testing the null hypothesis of the event having no impact using unaggregated security by security data. This approach is applied most commonly when there is total clustering, that is, there is an event on the same day for a number of firms. The basic approach is an application of a multivariate regression model with dummy variables for the event date. This approach is developed in the papers of Katherine Schipper and Rex Thompson (1983; 1985) and Daniel Collins and Warren Dent (1984). The advantage of the approach is that, unlike the portfolio

approach, an alternative hypothesis where some of the firms have positive abnormal returns and some of the firms have negative abnormal returns can be accommodated. However, in general the approach has two drawbacks—frequently the test statistic will have poor finite sample properties except in special cases and often the test will have little power against economically reasonable alternatives. The multivariate framework and its analysis is similar to the analysis of multivariate tests of asset pricing models. MacKinlay (1987) provides analysis in that context.

6. *Modifying the Null Hypothesis*

Thus far the focus has been on a single null hypothesis—that the given event has no impact on the behavior of the returns. With this null hypothesis either a mean effect or a variance effect will represent a violation. However, in some applications one may be interested in testing for a mean effect. In these cases, it is necessary to expand the null hypothesis to allow for changing (usually increasing) variances. To allow for changing variance as part of the null hypothesis, it is necessary to eliminate the reliance on the past returns to estimate the variance of the aggregated cumulative abnormal returns. This is accomplished by using the cross section of cumulative abnormal returns to form an estimator of the variance for testing the null hypothesis. Ekkehart Boehmer, Jim Musumeci, and Annette Poulsen (1991) discuss methodology to accommodate changing variance.

The cross sectional approach to estimating the variance can be applied to the average cumulative abnormal return ($\overline{CAR}(\tau_1,\tau_2)$). Using the cross-section to form an estimator of the variance gives

$$\mathrm{var}(\overline{CAR}(\tau_1,\tau_2))$$

$$= \frac{1}{N^2} \sum_{l=1}^{N} (CAR_l(\tau_1,\tau_2)$$

$$- \overline{CAR}(\tau_1,\tau_2))^2. \quad (21)$$

For this estimator of the variance to be consistent, the abnormal returns need to be uncorrelated in the cross-section. An absence of clustering is sufficient for this requirement. Note that cross-sectional homoskedasticity is not required. Given this variance estimator, the null hypothesis that the cumulative abnormal returns are zero can then be tested using the usual theory.

One may also be interested in the question of the impact of an event on the risk of a firm. The relevant measure of risk must be defined before this question can be addressed. One choice as a risk measure is the market model beta which is consistent with the Capital Asset Pricing Model being appropriate. Given this choice, the market model can be formulated to allow the beta to change over the event window and the stability of the risk can be examined. Edward Kane and Haluk Unal (1988) present an application of this idea.

7. Analysis of Power

An important consideration when setting up an event study is the ability to detect the presence of a non-zero abnormal return. The inability to distinguish between the null hypothesis and economically interesting alternatives would suggest the need for modification of the design. In this section the question of the likelihood of rejecting the null hypothesis for a specified level of abnormal return associated with an event is addressed. Formally, the power of the test is evaluated.

Consider a two-sided test of the null hypothesis using the cumulative abnormal return based statistic θ_1 from (20). It is assumed that the abnormal returns are uncorrelated across securities; thus the variance of $\overline{CAR}$ is $1/N^2 \sum_{l=1}^{N} \sigma_l^2(\tau_1,\tau_2)$ and N is the sample size. Because the null distribution of θ_1 is standard normal, for a two sided test of size α, the null hypothesis will be rejected if θ_1 is in the critical region, that is,

$$\theta_1 < c\left(\frac{\alpha}{2}\right) \text{ or } \theta_1 > c\left(1-\frac{\alpha}{2}\right)$$

where $c(x) = \phi^{-1}(x)$. $\phi(\cdot)$ is the standard normal cumulative distribution function (CDF).

Given the specification of the alternative hypothesis H_A and the distribution of θ_1 for this alternative, the power of a test of size α can be tabulated using the power function,

$$P(\alpha,H_A) = pr\left(\theta_1 < c\left(\frac{\alpha}{2}\right) \mid H_A\right)$$

$$+ pr\left(\theta_1 > c\left(1-\frac{\alpha}{2}\right) \mid H_A\right). \quad (22)$$

The distribution of θ_1 under the alternative hypothesis considered below will be normal. The mean will be equal to the true cumulative abnormal return divided by the standard deviation of $\overline{CAR}$ and the variance will be equal to one.

To tabulate the power one must posit economically plausible scenarios. The alternative hypotheses considered are four levels of abnormal returns, 0.5 percent, 1.0 percent, 1.5 percent, and 2.0 percent and two levels of the average variance for the cumulative abnormal return of a given security over the event period, 0.0004 and 0.0016. The

TABLE 2

Sample Size	Abnormal Return $\sigma = 0.02$				Abnormal Return $\sigma = 0.04$			
	.005	.010	.015	.020	.005	.010	.015	.020
1	0.06	0.08	0.12	0.17	0.05	0.06	0.07	0.08
2	0.06	0.11	0.19	0.29	0.05	0.06	0.08	0.11
3	0.07	0.14	0.25	0.41	0.06	0.07	0.10	0.14
4	0.08	0.17	0.32	0.52	0.06	0.08	0.12	0.17
5	0.09	0.20	0.39	0.61	0.06	0.09	0.13	0.20
6	0.09	0.23	0.45	0.69	0.06	0.09	0.15	0.23
7	0.10	0.26	0.51	0.75	0.06	0.10	0.17	0.26
8	0.11	0.29	0.56	0.81	0.06	0.11	0.19	0.29
9	0.12	0.32	0.61	0.85	0.07	0.12	0.20	0.32
10	0.12	0.35	0.66	0.89	0.07	0.12	0.22	0.35
11	0.13	0.38	0.70	0.91	0.07	0.13	0.24	0.38
12	0.14	0.41	0.74	0.93	0.07	0.14	0.25	0.41
13	0.15	0.44	0.77	0.95	0.07	0.15	0.27	0.44
14	0.15	0.46	0.80	0.96	0.07	0.15	0.27	0.44
15	0.16	0.49	0.83	0.97	0.08	0.16	0.29	0.46
16	0.17	0.52	0.85	0.98	0.08	0.16	0.31	0.49
17	0.18	0.54	0.87	0.98	0.08	0.17	0.32	0.52
18	0.19	0.56	0.89	0.99	0.08	0.18	0.34	0.54
19	0.19	0.59	0.90	0.99	0.08	0.19	0.36	0.56
20	0.20	0.61	0.92	0.99	0.08	0.19	0.37	0.59
25	0.24	0.71	0.96	1.00	0.09	0.20	0.39	0.61
30	0.28	0.78	0.98	1.00	0.10	0.24	0.47	0.71
35	0.32	0.84	0.99	1.00	0.11	0.28	0.54	0.78
40	0.35	0.89	1.00	1.00	0.11	0.32	0.60	0.84
45	0.39	0.92	1.00	1.00	0.12	0.35	0.66	0.89
50	0.42	0.94	1.00	1.00	0.13	0.39	0.71	0.92
60	0.49	0.97	1.00	1.00	0.14	0.42	0.76	0.94
70	0.55	0.99	1.00	1.00	0.16	0.49	0.83	0.97
80	0.61	0.99	1.00	1.00	0.18	0.55	0.88	0.99
90	0.66	1.00	1.00	1.00	0.20	0.61	0.92	0.99
100	0.71	1.00	1.00	1.00	0.22	0.66	0.94	1.00
120	0.78	1.00	1.00	1.00	0.24	0.71	0.96	1.00
140	0.84	1.00	1.00	1.00	0.28	0.78	0.98	1.00
160	0.89	1.00	1.00	1.00	0.32	0.84	0.99	1.00
180	0.92	1.00	1.00	1.00	0.35	0.89	1.00	1.00
200	0.94	1.00	1.00	1.00	0.39	0.92	1.00	1.00
					0.42	0.94	1.00	1.00

Power of event study methodology for test of the null hypothesis that the abnormal return is zero. The power is reported for a two-sided test using θ_1 with a size of 5 percent. The sample size is the number of event observations included the study and σ is the square root of the average variance of the abnormal return across firms.

sample size, that is the number of securities for which the event occurs, is varied from one to 200. The power for a test with a size of 5 percent is documented. With $\alpha = 0.05$, the critical values calculated using $c(\alpha/2)$ and $c(1 - \alpha/2)$ are -1.96 and 1.96 respectively. Of course, in applications, the power of the test should be considered when selecting the size.
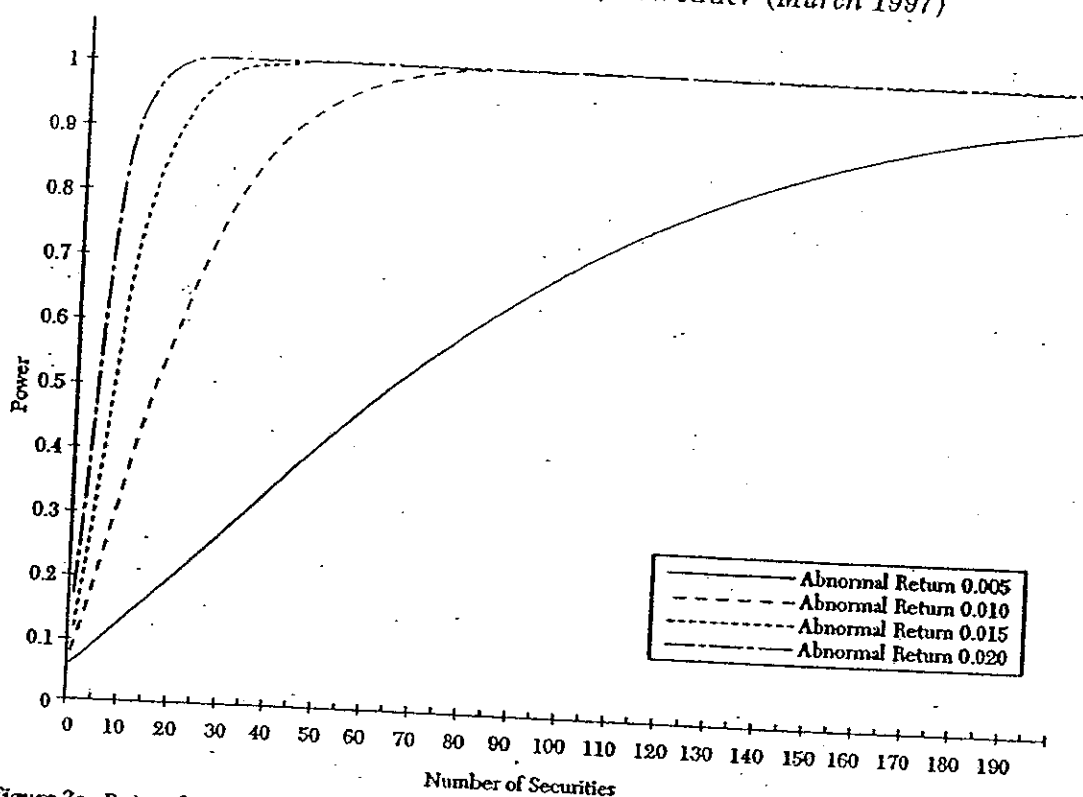
30
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Figure 3a. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, when the square root of the average variance of the abnormal return across firms is 2 percent.

The power results are presented in Table 2, and are plotted in Figures 3a and 3b. The results in the left panel of Table 2 and Figure 3a are for the case where the average variance is 0.0004. This corresponds to a cumulative abnormal return standard deviation of 2 percent and is an appropriate value for an event which does not lead to increased variance and can be examined using a one-day event window. In terms of having high power this is the best case scenario. The results illustrate that when the abnormal return is only 0.5 percent the power can be low. For example with a sample size of 20 the power of a 5 percent test is only 0.20. One needs a sample of over 60 firms before the power reaches 0.50. However, for a given sample size, increases in power

are substantial when the abnormal return is larger. For example, when the abnormal return is 2.0 percent the power of a 5 percent test with 20 firms is almost 1.00 with a value of 0.99. The general results for a variance of 0.0004 is that when the abnormal return is larger than 1 percent the power is quite high even for small sample sizes. When the abnormal return is small a larger sample size is necessary to achieve high power.

In the right panel of Table 2 and in Figure 3b the power results are presented for the case where the average variance of the cumulative abnormal return is 0.0016. This case corresponds roughly to either a multi-day event window or to a one-day event window with the event leading to increased variance
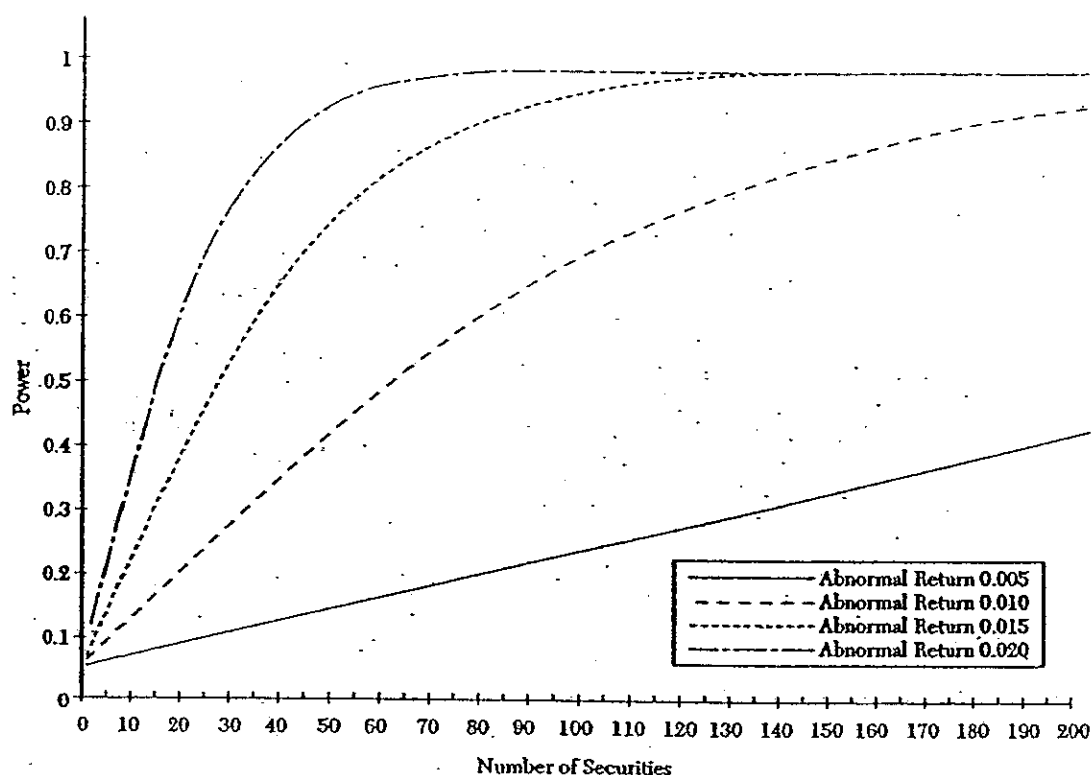
Figure 3b. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, when the square root of the average variance of the abnormal return across firms is 4 percent.

which is accommodated as part of the null hypothesis. When the average variance of the CAR is increased from 0.0004 to 0.0016 there is a dramatic power decline for a 5 percent test. When the CAR is 0.5 percent the power is only 0.09 with 20 firms and is only 0.42 with a sample of 200 firms. This magnitude of abnormal return is difficult to detect with the larger variance. In contrast, when the CAR is as large as 1.5 percent or 2.0 percent the 5 percent test is still has reasonable power. For example, when the abnormal return is 1.5 percent and there is a sample size of 30 the power is 0.54. Generally if the abnormal return is large one will have little difficulty rejecting the null hypothesis of no abnormal return.

In the preceding analysis the power is

considered analytically for the given distributional assumptions. If the distributional assumptions are inappropriate then the results may differ. However, Brown and Warner (1985) consider this possible difference and find that the analytical computations and the empirical power are very close.

It is difficult to make general conclusions concerning the adequacy of the ability of event study methodology to detect non-zero abnormal returns. When conducting an event study it is best to evaluate the power given the parameters and objectives of the study. If the power seems sufficient then one can proceed, otherwise one should search for ways of increasing the power. This can be done by increasing the sample size, shortening the event window, or by

developing more specific predictions to test.

8. Nonparametric Tests

The methods discussed to this point are parametric in nature, in that specific assumptions have been made about the distribution of abnormal returns. Alternative approaches are available which are nonparametric in nature. These approaches are free of specific assump- tions concerning the distri- bution of returns. Common nonparamet- ric tests for event studies are the sign test and the rank test. These tests are dis- cussed next.

The sign test, which is based on the sign of the abnormal return, requires that the abnormal returns (or more gen- erally cumulative abnormal returns) are independent across securities and that the expected proportion of positive ab- normal returns under the null hypothesis is 0.5. The basis of the test is that, under the null hypothesis, it is equally probable that the CAR will be positive or nega- tive. If, for example, the null hypothesis is that there is a positive abnormal re- turn associated with a given event, the null hypothesis is $H_0: p \le 0.5$ and the al- ternative is $H_A: p > 0.5$ where $p = pr[CAR_i \ge 0.0]$. To calculate the test sta- tistic we need the number of cases where the abnormal return is positive, N^+, and the total number of cases, N. Letting θ_2 be the test statistic,

$$\theta_2 = \left[\frac{N^+}{N} - 0.5 \right] \frac{\sqrt{N}}{0.5} \sim N(0,1). \qquad (23)$$

This distributional result is asymptotic. For a test of size $(1 - \alpha)$, H_0 is rejected if $\theta_2 > \Phi^{-1}(\alpha)$.

A weakness of the sign test is that it may not be well specified if the distri- bution of abnormal returns is skewed as can be the case with daily data. In re- sponse to this possible shortcoming,

Charles Corrado (1989) proposes a non- parametric rank test for abnormal per- formance in event studies. A brief de- scription of his test of no abnormal return for event day zero follows. The framework can be easily altered for more general tests.

Drawing on notation previously intro- duced, consider a sample of L_2 abnormal returns for each of N securities. To im- plement the rank test, for each security it is necessary to rank the abnormal re- turns from one to L_2. Define $K_{i\tau}$ as the rank of the abnormal return of security i for event time period τ. Re- call, τ ranges from $T_1 + 1$ to T_2 and $\tau = 0$ is the event day. The rank test uses the fact that the expected rank of the event day is $(L_2 + 1)/2$ under the null hypothe- sis. The test statistic for the null hy- pothesis of no abnormal return on event day zero is

$$\theta_3 = \frac{1}{N} \sum_{i=1}^{N} \left(K_{i0} - \frac{L_2 + 1}{2} \right) / s(K) \qquad (24)$$

where

$$s(K) = \sqrt{\frac{1}{L_2} \sum_{\tau=T_1+1}^{T_2} \left(\frac{1}{N} \sum_{i=1}^{N} \left(K_{i\tau} - \frac{L_2+1}{2} \right) \right)^2}. \qquad (25)$$

Tests of the null hypothesis can be im- plemented using the result that the as- ymptotic null distribution of θ_3 is stan- dard normal. Corrado (1989) includes further discussion of details of this test.

Typically, these nonparametric tests are not used in isolation but in conjunc- tion with the parametric counterparts. Inclusion of the nonparametric tests pro- vides a check of the robustness of con- clusions based on parametric tests. Such a check can be worthwhile as illustrated by the work of Cynthia Campbell and Charles Wasley (1993). They find that for NASDAQ stocks daily returns the nonparametric rank test provides more reliable inferences than do the standard parametric tests.

9. Cross-Sectional Models

Theoretical insights can result from examining the association between the magnitude of the abnormal return and characteristics specific to the event observation. Often such an exercise can be helpful when multiple hypotheses exist for the source of the abnormal return. A cross-sectional regression model is an appropriate tool to investigate this association. The basic approach is to run a cross-sectional regression of the abnormal returns on the characteristics of interest.

Given a sample of N abnormal return observations and M characteristics, the regression model is:

$$AR_j = \delta_0 + \delta_1 x_{1j} + \cdots + + \delta_M x_{Mj} + \eta_j \quad (26)$$

$$E(\eta_j) = 0 \quad (27)$$

where AR_j is the jth abnormal return observation, $x_{mj}, m = 1, \ldots, M$, are M characteristics for the jth observation and η_j is the zero mean disturbance term that is uncorrelated with the x's. $\delta_m, m = 0, \ldots, M$ are the regression coefficients. The regression model can be estimated using OLS. Assuming the η_j's are cross-sectionally uncorrelated and homoskedastic, inferences can be conducted using the usual OLS standard errors. Alternatively, without assuming homoskedasticity, heteroskedasticity-consistent t-statistics using standard errors can be derived using the approach of Halbert White (1980). The use of heteroskedasticity-consistent standard errors is advisable because there is no reason to expect the residuals of (26) to be homoskedastic.

Paul Asquith and David Mullins (1986) provide an example of this cross-sectional approach. The two day cumulative abnormal return for the announcement of an equity offering is regressed on the size of the offering as a percentage of the value of the total equity of the firm and on the cumulative abnormal re-

turn in the eleven months prior to the announcement month. They find that the magnitude of the (negative) abnormal return associated with the announcement of equity offerings is related to both these variables. Larger pre-event cumulative abnormal returns are associated with less negative abnormal returns and larger offerings are associated with more negative abnormal returns. These findings are consistent with theoretical predictions which they discuss.

Issues concerning the interpretation of the results can arise with the cross-sectional regression approach. In many situations, the event window abnormal return will be related to firm characteristics not only through the valuation effects of the event but also through a relation between the firm characteristics and the extent to which the event is anticipated. This can happen when investors rationally use the firm characteristics to forecast the likelihood of the event occurring. In these cases, a linear relation between the valuation effect of the event and the firm characteristic can be hidden. Paul Malatesta and Thompson (1985) and William Lanen and Thompson (1988) provide examples of this situation.

Technically, with the relation between the firm characteristics and the degree of anticipation of the event introduces a selection bias. The assumption that the regression residual is uncorrelated with the regressors breaks down and the OLS estimators are inconsistent. Consistent estimators can be derived by explicitly incorporating the selection bias. Sankarshan Acharya (1988) and B. Espen Eckbo, Vojislav Maksimovic, and Joseph Williams (1990) provide examples of this approach. N. R. Prabhala (1995) provides a good discussion of this problem and the possible solutions. He argues that, despite an incorrect specification, under weak conditions, the OLS ap-
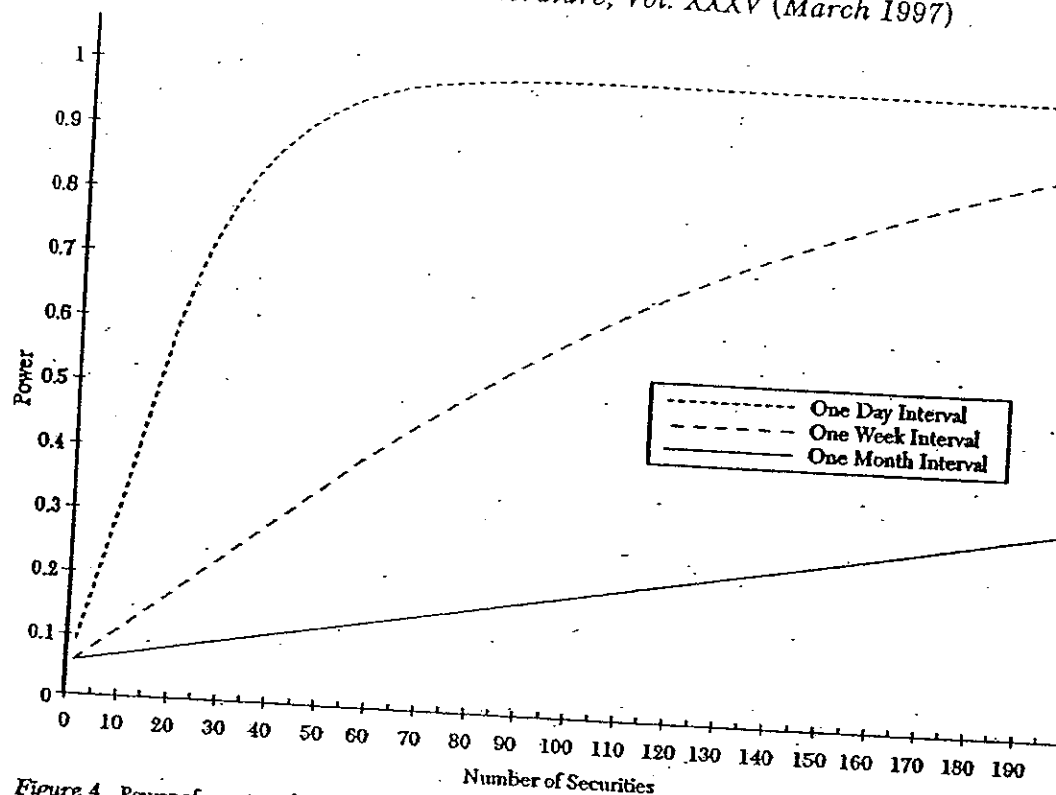
Figure 4. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, for different sampling intervals, when the square root of the average variance of the abnormal return across firms is 4 percent for the daily interval. Size of test is 5 percent.

proach can be used for inferences and that the *t*-statistics can be interpreted as lower bounds on the true significance level of the estimates.

10. Other Issues

A number of further issues often arise when conducting an event study. These issues include the role of the sampling interval, event date uncertainty, robustness, and some additional biases.

A. Role of Sampling Interval

Stock return data is available at different sampling intervals, with daily and monthly intervals being the most common. Given the availability of various intervals, the question of the gains of using more frequent sampling arises. To address this question one needs to consider the power gains from shorter intervals. A comparison of daily versus monthly data is provided in Figure 4. The power of the test of no event effect is plotted against the alternative of an abnormal return of one percent for 1 to 200 securities. As one would expect given the analysis of Section 7, the decrease in power going from a daily interval to a monthly interval is severe. For example, with 50 securities the power for a 5 percent test using daily data is 0.94, whereas the power using weekly and monthly data is only 0.35 and 0.12 respectively. The clear message is that there is a substantial payoff in terms of increased power from reducing the sampling inter-

val. Dale Morse (1984) presents detailed analysis of the choice of daily versus monthly data and draws the same conclusion.

A sampling interval of one day is not the shortest interval possible. With the increased availability of transaction data, recent studies have used observation intervals of duration shorter than one day. However, the net benefit of intervals less than one day is unclear as some complications are introduced. Discussion of using transaction data for event studies is included in the work of Michael Barclay and Robert Litzenberger (1988).

B. *Inferences with Event-Date Uncertainty*

Thus far it is assumed that the event date can be identified with certainty. However, in some studies it may be difficult to identify the exact date. A common example is when collecting event dates from financial publications such as the *Wall Street Journal*. When the event announcement appears in the paper one can not be certain if the market was informed prior to the close of the market the prior trading day. If this is the case then the prior day is the event day, if not then the current day is the event day. The usual method of handling this problem is to expand the event window to two days—day 0 and day +1. While there is a cost to expanding the event window, the results in Section 6 indicated that the power properties of two day event windows are still good suggesting that the costs are worth bearing rather than to take the risk of missing the event.

Clifford Ball and Walter Torous (1988) have investigated the issue. They develop a maximum likelihood estimation procedure which accommodates event date uncertainty and examine results of their explicit procedure versus the informal procedure of expanding the event window. The results indicates that the informal procedure works well and there is little to gain from the more elaborate estimation framework.

C. *Robustness*

The statistical analysis of Sections 4, 5, and 6 is based on assumption that returns are jointly normal and temporally independently and identically distributed. In this section, discussion of the robustness of the results to departures from this assumption is presented. The normality assumption is important for the exact finite sample results to hold. Without assuming normality, all results would be asymptotic. However, this is generally not a problem for event studies because for the test statistics, convergence to the asymptotic distributions is rather quick. Brown and Warner (1985) provide discussion of this issue.

D. *Other Possible Biases*

A number of possible biases can arise in the context of conducting an event study. Nonsynchronous trading can introduce a bias. The nontrading or nonsynchronous trading effect arises when prices, are taken to be recorded at time intervals of one length when in fact they are recorded at time intervals of other possibly irregular lengths. For example, the daily prices of securities usually employed in event studies are generally "closing" prices, prices at which the last transaction in each of those securities occurred during the trading day. These closing prices generally do not occur at the same time each day, but by calling them "daily" prices, one is implicitly and incorrectly assuming that they are equally spaced at 24-hour intervals. This nontrading effect induces biases in the moments and co-moments of returns.

The influence of the nontrading effect on the variances and covariances of individual stocks and portfolios naturally feeds into a bias for the market model

beta. Myron Scholes and Williams (1977) present a consistent estimator of beta in the presence of nontrading based on the assumption that the true return process is uncorrelated through time. They also present some empirical evidence which shows the nontrading-adjusted beta estimates of thinly traded securities to be approximately 10 to 20 percent larger than the unadjusted estimates. However, for actively traded securities, the adjustments are generally small and unimportant.

Prem Jain (1986) considers the influence of thin trading on the distribution of the abnormal returns from the market model with the beta estimated using the Scholes-Williams approach. When comparing the distribution of these abnormal returns to the distribution of the abnormal returns using the usual OLS betas finds that the differences are minimal. This suggests that in general the adjustments for thin trading are not important.

The methodology used to compute the cumulative abnormal returns can induce an upward bias. The bias arises from the observation by observation rebalancing to equal weights implicit in the calculation of the aggregate cumulative abnormal return combined with the use of transaction prices which can represent both the bid and the offer side of the market. Marshall Blume and Robert Stambaugh (1983) analyze this bias and show that it can be important for studies using low market capitalization firms which have, in percentage terms, wide bid offer spreads. In these cases the bias can be eliminated by considering cumulative abnormal returns which represent buy and hold strategies.

11. Concluding Discussion

In closing, examples of event study successes and limitations are presented. Perhaps the most successful applications

have been in the area of corporate finance. Event studies dominate the empirical research in this area. Important examples include the wealth effects of mergers and acquisitions and the price effects of financing decisions by firms. Studies of these events typically focus on the abnormal return around the date of first announcement.

In the 1960s there was a paucity of empirical evidence on the wealth effects of mergers and acquisitions. For example, Henry Manne (1965) discusses the various arguments for and against mergers. At that time the debate centered on the extent to which mergers should be regulated in order to foster competition in the product markets. Manne argued that mergers represent a natural outcome in an efficiently operating market for corporate control and consequently provide protection for shareholders. He downplayed the importance of the argument that mergers reduce competition. At the conclusion of his article Manne suggested that the two competing hypotheses for mergers could be separated by studying the price effects of the involved corporations. He hypothesized that, if mergers created market power, one would observe price increases for both the target and acquirer. In contrast, if the merger represented the acquiring corporation paying for control of the target, one would observe a price increase for the target only and not for the acquirer. However, Manne concludes, in reference to the price effects of mergers, that "no data are presently available on this subject."

Since that time an enormous body of empirical evidence on mergers and acquisitions has developed which is dominated by the use of event studies. The general result is that, given a successful takeover, the abnormal returns of the targets are large and positive and the abnormal returns of the acquirer are close

to zero. Gregg Jarrell and Poulsen (1989) document that the average abnormal return for target shareholders exceeds 20 percent for a sample of 663 successful takeovers from 1960 to 1985. In contrast the abnormal returns for acquirers is close to zero. For the same sample, Jarrell and Poulsen find an average abnormal return of 1.14 percent for acquirers. In the 1980s they find the average abnormal return is negative at -1.10 percent. Eckbo (1983) explicitly addresses the role of increased market power in explaining merger related abnormal returns. He separates mergers of competing firms from other mergers and finds no evidence that the wealth effects for competing firms are different. Further, he finds no evidence that rivals of firms merging horizontally experience negative abnormal returns. From this he concludes that reduced competition in the product market is not an important explanation for merger gains. This leaves competition for corporate control a more likely explanation. Much additional empirical work in the area of mergers and acquisitions has been conducted. Michael Jensen and Richard Ruback (1983) and Jarrell, James Brickley, and Netter (1988) provide detailed surveys of this work.

A number of robust results have been developed from event studies of financing decisions by corporations. When a corporation announces that it will raise capital in external markets there is, on average, a negative abnormal return. The magnitude of the abnormal return depends on the source of external financing. Asquith and Mullins (1986) find for a sample of 266 firms announcing an equity issue in the period 1963 to 1981 the two day average abnormal return is -2.7 percent and on a sample of 80 firms for the period 1972 to 1982 Wayne Mikkelson and Megan Partch (1986) find the two day average abnormal return is

-3.56 percent. In contrast, when firms decide to use straight debt financing, the average abnormal return is closer to zero. Mikkelson and Partch (1986) find the average abnormal return for debt issues to be -0.23 percent for a sample of 171 issues. Findings such as these provide the fuel for the development of new theories. For example, in this case, the findings motivate the pecking order theory of capital structure developed by Stewart Myers and Nicholas Majluf (1984).

A major success related to those in the corporate finance area is the implicit acceptance of event study methodology by the U.S. Supreme Court for determining materiality in insider trading cases and for determining appropriate disgorgement amounts in cases of fraud. This implicit acceptance in the 1988 Basic, Incorporated v. Levinson case and its importance for securities law is discussed in Mitchell and Netter (1994).

There have also been less successful applications. An important characteristic of a successful event study is the ability to identify precisely the date of the event. In cases where the event date is difficult to identify or the event date is partially anticipated, studies have been less useful. For example, the wealth effects of regulatory changes for affected entities can be difficult to detect using event study methodology. The problem is that regulatory changes are often debated in the political arena over time and any accompanying wealth effects generally will gradually be incorporated into the value of a corporation as the probability of the change being adopted increases.

Larry Dann and Christopher James (1982) discuss this issue in the context of the impact of deposit interest rate ceilings for thrift institutions. In their study of changes in rate ceilings, they decide not to consider a change in 1973 because it was due to legislative action. Schipper

and Thompson (1983, 1985) also encounter this problem in a study of merger related regulations. They attempt to circumvent the problem of regulatory changes being anticipated by identifying dates when the probability of a regulatory change being passed changes. However, they find largely insignificant results leaving open the possibility the of absence of distinct event dates as the explanation of the lack of wealth effects.

Much has been learned from the body of research based on the use of event study methodology. In a general context, event studies have shown that, as would be expected in a rational marketplace, prices do respond to new information. As one moves forward, it is expected that event studies will continue to be a valuable and widely used tool in economics and finance.

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